

DISCUSSION BEFORE THE WIRELESS SECTION, 6TH MARCH, 1929.

Mr. T. L. Eckersley: Ten or fifteen years ago the idea of holding two or more stations with the same frequency within 2 beats in 1 million would have seemed quite impossible, but this has been achieved. In attempting to explain the theoretical working of the system the authors have not, I think, laid sufficient stress on the reason why absolute synchronism is necessary. The trouble is that if the field strength due to a distant station is comparable in intensity with that of the local station it is desired to hear, distortion may result, even when the two are practically in synchronism. The causes of this distortion have been explained quite clearly by the authors; but much worse distortion, or rather drowning by the beat-tone between the carrier waves of the distant station and the local station, will occur if the two stations are not actually in synchronism. For instance, the carrier wave of the local station has side-waves which are probably of the order of 20 per cent of the carrier of the local station. If the distant station has a carrier of the same order of intensity—i.e. 20 per cent, which is the limit laid down by the B.B.C. for single-wave working—the beat-tone caused by the local and the distant carrier forming a heterodyne note, and the beat-tone between the carrier and side-waves of the local station, will be of the same order of intensity. The result will be that the speech or music will be drowned by a continuous heterodyne note. If, however, the carrier wave of the disturbing station is imposed exactly on top of that of the local station, the slight variation of intensity of 20 per cent produced by variations in phase between that and the carrier wave of the local station will have a negligible effect. It does not matter whether the resultant carrier is 20 per cent higher or less. The result is that there will be no distortion. That appears to me to be one of the reasons why it is possible, with accurate synchronism, to obtain a very much more extensive service area than would otherwise be the case. Without accurate synchronism, serious interference may be caused by heterodyne notes from the distant station. The authors have been somewhat optimistic in estimating the strength of the reflected ray from the Heaviside layer. During the last two or

three months we have made a considerable number of actual measurements of the field strength due to the down-coming waves of broadcasting stations at very considerable distances. I have plotted a curve which directly compares our results with those of the authors. This curve represents the reflection coefficient of the Heaviside layer. The authors have assumed a flat rate of reflection of about 10 per cent. This does not agree with what we found in actual practice. We found a variation of reflection coefficient with distance, although the mean value is approximately the same as that given by the authors. But it is by no means constant. Working from this result, and comparing the actual signal strength received at a distance, I find that our signal intensities are of the order of $1\frac{1}{2}$ to 2 times as great as those given by the authors, possibly because of a different method used in defining the reflection coefficient. The authors' method of verifying Lord Rayleigh's theory of probabilities is very ingenious, but perhaps a little unnecessary considering how widely known his results are, at least among physicists. The object of the authors was, I presume, to compare the *average* disturbing effect of n stations with one. The maximum effect of the n stations cannot have been in question, for it is obviously n times as great as one. The maximum effect over a given time is indefinite, since it depends on the time interval over which the effects occur. The average disturbing effect is that which concerns the listeners; this is given precisely by Lord Rayleigh's analysis and is equal to \sqrt{n} . The maximum effect over a short interval is a good approximation to this case, but exaggerates the effect for the smaller values of n . The curve obtained, however, shows a good approximation to the form \sqrt{n} for the larger values of n .

Mr. H. J. Lucas: I desire to comment only on the question of the standardization and control of the common wave frequency. Apparently the onus of choice as to the most suitable method was placed upon Dr. Dye. None is likely to dispute his decision on such a point, and at the time the reasons which governed his decision were sound. The piezo-electric quartz crystal

was certainly not a serious competitor. Frequency errors of the order of 1 part in 2 000 or 3 000 had to be allowed for. This condition no longer obtains. A consideration of the properties of steel and quartz does not suggest any inherent disability of the latter as compared with the former as a material for a frequency standard. Indeed, when research on the quartz frequency standard in the matter of selection of material, mounting, adjustment and loading, amounts to that expended on the development of the elinvar fork standard, a definitely improved standard may with some confidence be anticipated. In the meantime, recent American practice indicates that, as far as the United States is concerned, quartz as a frequency standard is an accepted fact. Two typical instances are (1) the adoption of quartz as the United States primary naval standard,* and (2) a proposal to check the earth's speed of rotation by means of a quartz standard.† Also, in this country certain research work ‡ has contributed to establishing quartz on a reliable basis as a frequency standard. I have had three nominal 100 000 cycles per sec. standards constructed on lines indicated by the last-mentioned research and have had them under observation for two years. Their values are 100 002, 100 003.5 and 99 995 cycles per sec. No variation approaching 1 part in 100 000 between them has been detected up to the present. A rigorous investigation of their permanence of accuracy is at present in progress. In the matter of application, a similar type of crystal is used to drive an electrical system giving a range of standardized frequencies from 1 to 6 000 kilocycles per sec. in an analogous manner to Dr. Dye's multivibrator but with an extended range. Enough has been said to indicate that if the B.B.C. propose to extend common-wave working, they might consider quartz as an alternative method to the fork. Further points in favour of quartz are: (1) The temperature coefficient is negligibly small for suitably chosen samples and cuts of quartz; the standards previously referred to are of the order of 2 parts in 1 million per degree C. Thus thermostatic control is unnecessary and an appreciable saving in equipment effected. (2) The standard is transferred from the low-frequency to the high-frequency belt of the spectrum. If frequency doubling is still considered desirable, the 9 stages used by the authors may be reduced to 3. A further simplification could be effected by utilizing the method referred to as an application, instead of frequency doubling. In this case the appropriate harmonic is selected, amplified and used as a "drive." It might be asked why frequency-doubling, etc., could not be avoided by using a crystal of the desired frequency. The answer is that accurately standardized quartz crystals cannot at present be produced at much higher frequencies than 100 kilocycles per sec. Another minor difficulty would be perfect "pairing" of the crystals. This could not be achieved. Experience suggests that 5 parts in 1 million would be the best that could be obtained.

Mr. R. H. Barfield: In mentioning the inter-

ference between the two waves of the same length, the authors did not state whether they were referring to the electric or the magnetic field of the wave. The nodes will be situated at different places according to which of these fields is under consideration. If the two transmitters are not in line with the receiver, it should be possible to eliminate one station by ordinary directional reception and, if they are in line, the same end could be achieved by use of the heart-shaped diagram. No doubt that point has been considered. Most people use open aerials for broadcast reception, and I suppose that is why the authors did not recommend directional reception to overcome the difficulty. It might also be possible, if only 2 stations are concerned, to prevent radiation in the unwanted direction either by means of a suitable directional transmitter or by having a transmitter with a very bad polar diagram—which would be a very good one for this purpose. I have made a few rough calculations and it seems that it would be possible, by erecting a tuned aerial of fairly reasonable dimensions close to the transmitting aerial, to produce an absolutely blind spot in the direction of the unwanted station which has the same wave-length. The figures I used were a wave-length of 300 m, an effective height of 30 m, and a distance of 50 m from the transmitter. With this the resistance would have to be about 25 ohms to produce a complete wipe-out in a line with the screening aerial. Have the authors carried out any experiments on those lines? They should be interesting. I do not understand how the distance D in col. 23 of Table 2 was obtained. Was it derived from the other figures or is it merely an arbitrary figure? I do not agree with a statement appearing on page 89. Mr. Munro and I have produced curves showing that frequency may be all-important in determining field strength in the region of a transmitter; in one case the curves show that a change of wave-length from 500 to 200 m reduces the field strength in the ratio of 9 to 1. Although the power radiated remains constant, yet the authors make the statement that "the field strength 5 km from a station will be practically the same on any wave-length between 200 and 500 m." That is a flat contradiction of our results.

Dr. R. L. Smith-Rose: I admire the audacity of the authors in starting out with such vague assumptions and building up quite elaborate calculations on the basis thereof, but I can see the justification of that procedure in the results obtained. Here was a difficulty to be faced and overcome if possible, and the amount of data available in wireless matters even to-day is so limited that the authors had to make rather wild assumptions and do the best with the available material.

Dr. E. H. Rayner: How would the authors propose to allocate stations on a very limited number of wave-lengths over a country of more or less indefinite area? So far they have simply discussed the question of single-wave-length working from a number of stations, and they admit, I presume, that it is not a perfect solution for a large country. A perfect solution might well be reached for a large country by having a limited number of separate wave-lengths, each being operated from a considerable number of stations, so as to ensure good service not only in towns but everywhere else.

* R. H. Worrall and R. B. Owens: "United States Navy Frequency Standard," *Proceedings of the Institute of Radio Engineers*, 1928, vol. 16, p. 778.
 † L. P. Wheeler and W. E. Bower: "Standard Frequency Oscillator," *ibid.*, 1928, vol. 16, p. 1035.
 ‡ *Journal I.E.E.*, 1928, vol. 66, p. 867; and *Proceedings of the Wireless Section*, 1928, vol. 3, p. 163.

Mr. J. F. Herd: I should like to query the use of the word "synchronization" to indicate the state of two stations working at precisely the same frequency. "Synchronization" would denote identity not merely of frequency but also of phase. I think the correct word should be "isochronism," unless the authors can invent or produce another word meaning "identity of frequency." In connection with nomenclature, also, I do not think one can speak of a "supersonic note." The authors refer to a "semicircular polar diagram." No doubt they mean a vertical polar diagram of the shape discussed in a recent paper read before the Wireless Section, but the reference, as it stands, is ambiguous and should be clarified for readers of the present paper.

Dr. D. W. Dye: Had I known that my advice to use a tuning fork as a fundamental standard upon which to build the radio frequencies used in broadcasting stations would entail the use of 9 stages with 25 valves in order to step up from 1 000 to 1 000 000 cycles per sec., I think I should have hesitated. I am still convinced, however, that the tuning fork is a standard of the very highest class. Whether it is likely ultimately to be superior to the quartz crystal as a fundamental frequency standard I am unable to say; but there is no doubt that the tuning fork, properly mounted and used, can be made to give the accuracy desired, as is apparent in the excellent results obtained. There is little doubt that the frequency variation shown in Fig. 10 is due to the action of the thermo-regulator. The rate of variation seems to be exactly what one might expect, and there is little doubt that it does not represent any kind of instability at all; it is merely the result of the temperature cycles which are occurring in the tuning forks. I was rather surprised to find that a mild-steel fork had been used instead of a fork made from elinvar, a nickel-iron alloy which has only a small frequency-temperature coefficient. Apparently the only reason given is that concerned with the damping. There is no doubt that the damping of the fork is the most fundamental property which determines the smallness of frequency variations in respect of all the associated circuits, but I am doubtful whether that damping is any less in mild steel than in other materials, particularly in view of the necessity for magnetizing in each case. I think that a good deal more work requires to be done on mechanical oscillators in which other methods of maintaining oscillation are used. At the National Physical Laboratory we have oscillators in which the drive is electrostatic. The bars can then be relieved from the magnetization which certainly increases damping. With regard to the method used to step up from the tuning-fork frequency to the radio frequency, I should like to refer to a very simple circuit which I described some time ago but have not seen in use or published elsewhere. It is extremely simple, consisting of a valve oscillator having a resonant anode circuit and a resonant grid circuit closely coupled and adjusted so that the anode circuit is approximately harmonic to the grid circuit. Such a valve system generates a fundamental and the harmonic only to a close approximation, gives a considerable amount of power and does not make use of distortion by rectification. A cascade system built up of such units should possess very great

stability. With regard to the first section of the paper, in which the question of the standing waves is referred to, it has been my aim to try to produce standing waves with short waves in order to measure the velocity of propagation of these waves in free space. It seems to me that if we can, by absolutely synchronous driving of two stations, obtain perfect phase synchronism, it might be possible to measure the ground wave-length in the free space just above the surface by observations along the path. Such an experiment would be of great value.

Mr. H. E. Morrish: I am not quite clear as to how the problem of giving a service to the unfortunate inhabitants of the area between the two stations shown in Fig. 2 has been solved. I think the authors explained that they were serving about 1 200 000 inhabitants in their service area, but it might happen that some person residing in the mush area would take out a licence in advance. What is the guarantee that he would have a service? The incidence of population will not remain the same and a town may spring up in a mush area and demand a service there. I was intrigued by the solution of the problem of the 18 stations on the circumference of the circle shown, and I wondered what would be the effect had the 18 stations been situated at the centre of the circle. With the success of directional wireless beam stations, it occurred to me that if one could have a number of directional stations in the centre of an area and transmit from them simultaneously at the same frequency in different directions, one might get a better service distribution over a wider area than in the conditions shown in Fig. 2.

Captain P. P. Eckersley and Mr. A. B. Howe (*in reply*): The discussion is most helpful to us in suggesting new ideas and in affording us an opportunity to explain certain points which were possibly obscure in the paper.

Mr. T. L. Eckersley points out that synchronization, in eliminating audible heterodyne, does much to increase good service conditions near the station. This has considerable practical significance, inasmuch as it gives a further reason for abandoning the scheme of international common-wave working, in which waves are shared between stations in different countries and synchronization is therefore impractical.

Both Mr. Eckersley and Dr. Smith-Rose have cast some doubts on the scientific correctness of the assumptions which led to the calculation of the maximum value of the indirect ray. We were, however, at pains to indicate that we had no need to place too close a reliance on the figures. An error of 2 to 1, or even 3 to 1, makes little difference to the final conclusions. A great deal of experimental investigation needs to be done before the reflection constant of the Heaviside layer can be more accurately predicted, and it is suggested that this opens up a useful field for co-operative experiment. It would be necessary as a basis to assume the shape of the vertical polar diagram of the transmitting aerials used for the experiment. Probably even Mr. Eckersley's more accurate observations are, nevertheless, based on somewhat wide assumptions on this point.

It is quite true, as Mr. Barfield says, that if we are in the field of two synchronized stations it is possible

to get clear reception in a "mush" area provided that the influence of one station is eliminated by using a directional receiver. However, the case is not likely to be met with in practice; the practical usefulness of the scheme is that it allows several widely scattered stations to utilize only one channel and yet to give good service to a large number of listeners.

In answer to Mr. Morrish and Mr. Rayner, a "mush" area is filled up by a single station using another exclusive wave. Consider a hypothetical case of a circular country 200 miles in diameter with, say, 5 large towns located within its boundaries. The use of single-wave-length working for a national service would give to each town its own transmitter designed to provide the whole urban population with a strong signal, whilst a central high-power station would fill up the "mush" areas created in the country districts by the simultaneous operation of the 5 stations on the one wave-length. An excellent service throughout the country is thus given on only two channels, and yet every town is served by a strong signal sufficient to overcome such local electric disturbance as is frequently experienced in large cities. If it was desired by the authorities to give every listener alternative programmes according to the twin-wave station scheme outlined by one of the authors in a previous paper,* 4 wave-lengths would be sufficient. With older methods, and in this hypothetical case, the scheme would involve the occupancy of 12 channels. With larger countries the "mush-filling" station would be duplicated on a different wave-length, or a high-power long-wave station could be used. Undoubtedly, single-wave-length working provides the solution of many problems hitherto considered insoluble.

We have had an opportunity of discussing further with Mr. Lucas the relative value of crystals and tuning forks for independent synchronization. Mr. Lucas's long and patient work on this subject deserves wider recognition. He believes that with his arrangement of crystals accuracies of the order of 1 in a million may now be obtained, but he has not yet had an opportunity of testing their accuracy against, for instance, a tuning-fork standard. It is agreed, however, that at the time of the decision we were probably wiser to employ the tuning fork; and results since the installation of the apparatus certainly show that no other device could be expected to give greater satisfaction.

Further investigations must be made to account for the very small frequency variations shown in Fig. 10. Some experiments have been undertaken which indicate that, as Dr. Dye suggests, these might be due to the action of the thermo-regulator fan. The fan itself has caused induction in the fork-maintaining circuits owing to its changing local magnetic fields as well as slight variations in frequency due to its action in effecting irregular temperature variations round the fork. A slight and simple modification of the design eliminates this trouble.

Dr. Dye's remarks on the question of other types of standard lead one to think that the future may bring simpler types of drive suitable for synchronized stations; nevertheless, a start has been made and a great gain to the service has resulted. There may also be simpler

multiplier circuits, but none of those tried by us possessed the same inherent stability and ease of handling as the one chosen. A circuit very similar to that proposed by Dr. Dye was tried, but was discarded in favour of what appeared to be a more stable arrangement. It may seem rather a drawback to use so many valves; nevertheless, a valve is probably as reliable a piece of apparatus as any in existence, and our troubles come far more frequently from much older types of apparatus, for example, the electric motor or dynamo.

Turning to more detailed criticisms of the paper, we must apologize to Mr. Eckersley for proving Rayleigh's well-known physical theory.

As Mr. Herd points out, the term "synchronization" is, strictly speaking, a misnomer, and "isochronization" is clearly more accurate. However, it is doubtful whether strictly accurate terms will ever find their way into the more picturesque phraseology used by engineers; American engineers already speak of "synchronized service," and we doubt whether we could, with all the care in the world, have prevented the final adoption of the more descriptive but less accurate nomenclature. "Supersonic note" is indefensible. The term "semi-circular polar diagram" is commonly used to denote that the angular radiations from an aerial in the vertical plane can be expressed as $E \cos \alpha$, where α is the angle to the horizontal of any particular ray.

We cannot understand Mr. Barfield's remark about the difference between the interference pattern created by the magnetic and electric strains. Surely the nodes and antinodes occur at the same points in space, but the states of maximum and minimum strain are not simultaneous.

Since writing the paper we have had an opportunity of making further observations in actual practice, and it can be said with some confidence that the curves in Fig. 7 are reliable within the limits implied throughout the paper.

One previously unforeseen fact arises, however; as has been indicated, the paper was written in advance of a great deal of practical experience of the method described. The Bournemouth station has always been listened to on the Continent because it seemed to give an exceptional indirect-ray service. When it was admitted to the family of synchronized transmitters, it was expected that Continental listeners would complain that it no longer gave them the service to which they had been accustomed. Actually, instead of complaints we have received notice that the transmission is better and fades less: no one complained of "mush." It is still too early to make categorical statements; listeners accustomed to indirect-ray service are not notoriously critical of quality (from the very nature of the service they cannot be), but it is at least established that the distant service from many synchronized stations, so far from being impossible, may in certain circumstances be better than from a single station. This is suggestive, and indicates that several synchronized short-wave telephony transmitters may give better service than an individual station. All methods to equalize the quality and quantity of short-wavelength modulated waves at the receiver rely upon trying to piece together the fragments scattered haphazard from the Heaviside layer. Possibly such

* P. P. ECKERSLEY: "The Design and Distribution of Wireless Broadcasting Stations for a National Service," *Journal I.E.E.*, 1928, vol. 66, p. 501; and *Proceedings of the Wireless Section*, 1928, vol. 3, p. 108.

scattering would be less in the case of multiple transmission of the same pattern, and there would be a better chance for the receiver to piece together the scattered fragments of the original pattern.

Certain engineers, notably in America, have great hopes that indirect-ray service, whether by means of medium or short waves, may be greatly improved by the use of synchronization. If this comes about, single-wave-

length working bids fair to do more for certain kinds of broadcasting than the authors of this paper originally expected. In any case it is hoped that the preliminary survey is sufficiently complete to indicate an economic use of valuable wave-lengths for direct-ray service; it is left for the future to decide whether the method has other, possibly wider, fields of application in the art of broadcasting.
